CHOLERA.

39. *On the use of Cold Water in Cholera.*—By Dr. Hardwicke Shute, of Gloucester. (In a letter to the Central Board of Health.)—My attention was early directed to the fact, that no good, or rather as it appears to me most decided injury, was done by the administration of brandy, (or alcohol in any of its multifarious forms,) and even by the use of the stimulant emetics, when they were retained, as frequently happened in the advanced stages of collapse. In endeavouring to account for the deaths which commonly occurred in the course of a few hours, I was forcibly struck with the marked analogy which exists, as far as the state of the pulse and diminished animal heat are concerned, between the collapse of cholera and the impaired vital energy which results from starvation or long-continued exposure to excessive cold. Now it is an established fact in therapeutics, that the administering of a stimulus, disproportioned in strength or extent to the impaired vital energy of the system, is, under such circumstances, certain death; and that mortification of a frost-bitten extremity uniformly results from the hasty application of too great a degree of heat, or other stimulant, to the affected part. The treatment which I have adopted in the second and third stages of cholera is founded on the analogy already mentioned, and on the general principle that the necessity of diminishing or absolutely withdrawing every kind of stimulus is greater in proportion to the degree of collapse, or sinking of the vital powers.

The circumstance which particularly directed me to the remedy I employ, was the thirst, the excessive and inordinate desire on the part of the patient for water, for cold water more particularly, and I may almost say cold water exclusively; all other liquids being taken with reluctance, if not absolutely refused. I shall therefore state in general terms, that the free and unrestrained allowance of cold water, which, in the most marked cases of recovery, was taken to the extent of some gallons in a few hours, is the circumstance to which I wish particularly to direct your attention. If I add to this the abstraction of all kinds of stimulant remedies, both external and internal, even to the exclusion of friction, or the application of heat in any form, I have stated in general terms the whole plan of treatment, which, as I shall show, has succeeded in twelve out of fourteen, if not in twelve consecutive cases of the third stage of cholera. I am fully aware that the number of cases is too few to justify any thing like a general conclusion; but you will, I think, agree with me that they are not too few to justify a more extensive trial of the plan proposed. And it is with this view that I now address you; hoping that through your influence such trial will be made on a more extensive scale than is in the power of an individual.—I shall now mention some particulars which appear to me calculated to throw additional light on the plan of treatment I propose, it being understood that my observations apply to the *second* and *third*, particularly to the *third* stage of cholera, "when the pulse at the wrist has ceased, or become almost imperceptible."

The windows of the cholera hospital in Gloucester are large and numerous in proportion to the size of the apartments, and are open night and day. The doors open immediately into the garden, and are not suffered to be closed, so that the patients may be considered as living in the open air. I may add that the fires are purposely kept so low as to influence as little as possible the temperature of the room. The covering of the patients is confined to a light blanket and rug; and it seldom happens that some part of the body, particularly the breast and shoulders, is not constantly exposed. Under these circumstances, a pint of cold water is offered to the patient, and very frequently two-thirds of this are taken at a draught. In what I consider the most favourable cases, vomiting is almost immediately produced, and the patient in two or three minutes again calls for and eagerly drinks the same quantity with the same result. This is often continued for hours, until gallons of water have been taken, and the greatest proportion, (but I conceive not all,) rejected by vomiting. In other cases the patient is too insensible to ask for the water; and under these circumstances, it is offered every ten minutes or quarter of an hour, and most commonly drank with avidity. If gruel or tea be offered, the patient most frequently refuses it, and generally speaking no kind of nutriment is taken in any form until the period of convalescence. I consider it of great consequence that the vital powers should be restored as gradually as possible; and it is of importance to remark, that the progress towards recovery has been in all cases remarkably gradual and uniform. In the first six or eight hours no amendment can be observed except some diminution in the purple hue of the extremities; in the next six or eight hours there is a manifest improvement in the countenance of the patient, and more disposition to sleep, but often no restoration of the pulse or increase of temperature. In some cases the pulse has not been perceptible for twenty-four or thirty-six hours. From this period the pulse, the animal heat, and the secretions are very gradually restored, and at the end of forty-eight hours, or the third day from the commencement of the treatment proposed, the patient is convalescent, and in all cases without consecutive fever. I mention these circumstances particularly, in order that the practitioner may not become impatient. He should, in my opinion, be satisfied, and make not the least alteration in the plan proposed as long as the patient is merely not getting worse.

It would be inconsistent with the professed object of this memorial to offer any remarks upon the pathology of cholera, or upon the *modus operandi* of the treatment here recommended. But I cannot refrain from briefly suggesting, first, the importance of knowing;—that in the collapsed stage of cholera, cold may be extensively applied to the coats of the stomach without diminution, to use the most cautious term, of the vital energy. Secondly, that cold so applied has a manifest tendency to check the serous secretion, or perhaps, more correctly speaking, excretion or exudation of serum, which characterizes the disease. Thirdly, the acknowledged effect of vomiting in checking diarrhœa, equalizing the circulation, and emptying the vessels of the liver, and consequently the importance of not checking a natural action which has a conservative tendency. Fourthly, the probable effect of the liquid absorbed by the stomach in restoring the fluidity of the blood; and the presumption that the saline treatment, both as applied to the stomach and by its use in venous injection, may owe its effect as much, probably more, to the fluid itself, than to the ingredients it contains. Fifthly, the great importance of the arterial circulation being restored as gradually as possible, and the fact of convalescence taking place without the consecutive fever, which so often proves fatal in the *fourth* stage—the stage of reäction.

This method of treatment I have used with success in twelve of fourteen cases, and I may say in twelve consecutive cases. The first case in which it was tried and proved successful, occurred about six weeks ago, soon after the first appearance of the disease in this city. In the four first cases of the disease which came under my notice, internal and external stimulants were liberally administered. The mustard emetic and bleeding were also had recourse to, and every patient died in less than twelve, one in six hours. I determined therefore, (having understood from my professional brethren that the saline treatment had equally failed,) to take the first opportunity of watching the disease uninfluenced by remedies, with the hope that I might thus ascertain, if possible, the natural effects of the constitution, if any, to relieve the disease. In the cases already mentioned, I had observed that the stimulants were taken with great reluctance, and in one instance of extreme collapse, when the patient was apparently unconscious of every thing, I was much struck by an earnestly-expressed desire, that no fire should be made in the room, directions to that effect having been given in her hearing. In two of these cases the thirst was, I observed, excessive, and very distressing to the patient.

Soon after this, I was requested by a medical gentleman of this city to visit a female, fifteen years of age, whose father was at that time lying in the house dead of cholera. It was a marked case of collapse, with constant vomiting and purging of a fluid like rice-water. The pulse at that time, about one o'clock P. M. was scarcely perceptible. Calomel and opium had been previously given. I recommended a very liberal dose of opium, with various stimulant antispasmodics and olive-oil, and visited the patient again at seven o'clock that evening. The symptoms at that time were much aggravated—the vomiting and purging not at all relieved—the pulse imperceptible—expression of countenance cadaverous in the extreme. In despair, I recommended ten grains of musk to be given every hour, to the extent of two scruples; and left the patient with the impression, which was also that of the other medical gentleman in attendance who was with me, that she could not survive many hours. Having reflected upon the case, and still feeling anxious that relief, if possible, should be given, I returned in an hour with some green tea, and desired some to be made very strong and given. Whilst this was in preparation I visited the patient, and to relieve the thirst, which was excessive, presented a pint of cold water, the greater part of which was eagerly swallowed, and in less than a minute the whole was apparently rejected by vomiting. At this time I may observe, the symptoms were the same as last described. In two or three minutes after she *asked* for the water, which was drank, and immediately rejected as before. Having witnessed the continual repetition of this process for half an hour, (during which time the tea had been offered, and refused as soon as tasted,) without any increase of collapse, and with the satisfactory circumstance of a change in the voice, which had been highly characteristic of the disease, I left directions that cold water should be given to any extent which the patient might desire. I was informed the next morning that the patient had taken some gallons of water; had been constantly sick after each draught; but had not been sick, or asked for water the last hour. I also ascertained that the window, which opened immediately upon the bed, had not been closed the whole night. At this time there was no perceptible pulse or increase of temperature, but the countenance was, I thought, not quite so cadaverous in expression. The same plan was continued. In the middle of the day there was still no manifest improvement, but in the evening the pulse was perceptible. The next morning the pulse was decidedly improved, and on the following day the patient was convalescent, without any appearance of fever. If I have been tediously minute in the history of this case, I trust that it will be attributed to the great interest I must naturally feel in a case which terminated so satisfactorily, and which was the foundation of my future practice.

Soon after this, I was told by another medical gentleman that he was in despair, having lost seven cases of cholera in succession, and had just visited another, who, he supposed, would be dead before night; and that he had done nothing but ordered small doses of capsicum, with camphor mixture. I found this patient in a similar state to the last—recommended the unrestrained allowance of cold water—heard she was better the next day, and found her convalescent the following day, but with indications of fever. This circumstance, however, was accounted for by the fact of beer and cider, as well as cold water, having, through the prejudice of the attendant, been frequently given.

My experience, subsequent to the cases mentioned, has been confined to the Cholera Hospital in Gloucester, which was opened a fortnight ago, and placed under my superintendance, with the assistance of a resident surgeon. The first four cases sent to the hospital, which was then under my care, were all dead next morning. Since that period, of the forty-eight cases admitted, twenty-one have been discharged cured; eight I consider convalescent; eight are under treatment; and eleven have died. Of the eleven deaths, two might fairly be attributed to previous treatment, or other circumstances; three were in progress towards recovery, and relapsed from improper exertions on the part of the patients; two which were under three years of age, died under symptoms of cerebral congestion, after the pulse had been restored; one was more than sixty years of age, another about fifty, and extremely emaciated from previous disease or starvation; in one, the case was too advanced to admit of the plan being tried; and in another the patient was a notorious drunkard, and died a few hours after admission, with a gin bottle concealed about her person. If due consideration be given to these circumstances, the deaths cannot, I think, throw any discredit on the practice. I may add, that in all the cases great allowance must be made for the effect of previous treatment, as being most commonly directly opposite to my views of the disease. My experience hitherto justifies, in my opinion, the following conclusions, viz:—That in all cases the progress towards death is retarded by the water treatment; that when the irritability of the stomach is excessive or unimpaired, reäction will be established in a very great proportion of the cases; and that when the natural powers of the constitution are not unusually weak from extremes of age or other causes, such reäction will be followed by an early convalescence.—Any suggestion from those who are much more conversant with the disease than myself, will be received with gratitude, and any question answered to the utmost of my ability.—*Edin. Med. and Surg. Journ. October*, 1832.

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40. *Cholera without Intestinal Discharges.*—M. Girardin communicated to the Academy of Medicine of France, at their sitting on the 8th of January last, two fatal cases of cholera in which there had been no evacuations from the bowels. In one no post mortem examination was made, but in the other the intestines were found distended with the rice-gruel fluid; and, on tracing the rectum, it was ascertained that, for the extent of three or four inches, the gut was so much contracted, that a goose-quill could not be passed through it. Almost all the members agreed that these could not be regarded as cases of dry cholera, the discharges being prevented by the stricture of the rectum, and *none of them had met with any case of cholera in which the bowels were found empty after death.*

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41. *Saline Injections in Cholera.*—"The well-known fact that, in cholera, the serous and saline parts of the blood are rapidly thrown out upon the mucous surface of the bowels, naturally enough suggested the idea of supplying the loss by direct injection of a sero-saline kind into the veins. To throw such a fluid into the stomach or bowels, where it was already in superabundance, and at a time when all powers of absorption are suspended,* appears the wildest and most absurd scheme that ever was invented, as the result has shown. Contrary to expectation, the saline injections were not only borne with apparent safety, but, in almost every instance, produced a speedy, and as it were, a resuscitating effect on the whole system, restoring the pulse, the temperature, and the plumpness of the body. The prospect was too flattering to be realized. Such a gigantic power over disease—it might be said over death, was more than nature would permit, and accordingly we find two important drawbacks on the new remedy. Either the fluids run off by the bowels with more rapidity than ever—or the head or lungs become oppressed, and death takes place in a different manner than by cholera. Of both these events we have seen the most unequivocal and incontestable proofs. While therefore we bear testimony to the astonishing—the almost miraculous physiological effects of the saline injections, we are reluctantly compelled to bear witness to the pathological phenomena which succeed. We are grieved to say that, in our opinion, a remedy is yet to be sought for the collapse of the epidemic cholera. Till this be found, our only resource is vigilant attention to the first stage or diarrhœa, with which the disease almost always commences, when a milder form of the flux does not precede. We are now to notice some communications from practitioners on the subject of saline injections.

"Dr. Lawrie, of Glasgow, whose able work on cholera we have already noticed in this Journal, has stated the results of his observations in the Medical Gazette for June 7th, 1832. Following the directions of Dr. Latta, he injected from 70 to 150 ozs. within a few hours—and all his patients died, viz. six in number. In all of these temporary benefit was produced; 'but in several the fatal result seemed accelerated.' In one case he then tried the addition of albumen to the saline injection—in another the serum of bullock's blood—in a third, human serum—in a fourth, human blood—in two cases, small quantities of whiskey—in two, a few drops of laudanum—still *all* died. Dr. L. then began to suspect that the quantities injected were too large, and they were limited to 30 ounces at a time, thrown in very slowly, and carefully watching the effects. Under these precautions, four have recovered; 'but so many have died in despite of all precautions, so evidently injured by the practice, that I have now almost entirely laid it aside, as not only useless, but frequently hazardous.' Dr. L. injected 26 cases, 22 of which died! Dr. L. witnessed the phenomenon to which we have alluded—the drain of fluids from the bowels as fast as the injections were thrown into the veins. One patient made this remark—'as fast as you put water in my veins, it runs out by my stomach.'"—*Med. Chirur. Rev. Oct.* 1833.

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* This belief, which is pretty generally entertained, that the powers of absorption are entirely suspended in cholera, appears to us to be unsupported by any well-established fact.—*Ed.*

42. *Influence of Putrid Exhalations upon Cholera.*—M. Double has made a report to the Royal Academy of Medicine on this interesting subject. When the cholera appeared at Blois, a physician of that town saw with fear that the workmen had unpaved the streets for the purpose of cleansing them; he feared that the removed earth might give rise to fatal or choleric exhalations, an observation which he thought had already been made at Paris. In consequence of which he wrote to the prefect, and the latter transmitted his letters to the minister, who sent them to the Academy to decide the question. Numerous researches were necessary to arrive at this solution: M. Double devoted himself with the greatest care to this subject; he described all the work of this kind which had been done in Paris since the origin and during the existence of the cholera; he has followed them from street to street in the different sections of the city; he has given all their dimensions; he has pointed out the nature of the several strata of earth which have been dug out, and after the removal of the earth, he has shown that the inhabitants of the streets where the excavations were made, as well as the workmen employed, have been without comparison more healthy than all the others. He supports this remark, both by the recent experience of M. Parent-Duchatelet, and by the observation made at Montfaucon, where persons are occupied in the most unhealthy work, and where notwithstanding very few of the workmen are effected. From these facts M. Double concludes that the exhalations which escape from animal matters are not near so dangerous as have heretofore been supposed, and that, even during an epidemic, the removal of earth which increases these exhalations can be made without danger; but that this however, should not restrain the administration, when peculiar circumstances should force it to defer enterprises of this nature. After an animated discussion, the Academy decided that the conclusion of this report should not be expressed in terms decidedly affirmative. However, the final drawing up of the report was entrusted to M. Double, and the report and his conclusions were put to the vote and were adopted.—*Archives Générales, September,* 1832.

CHEMISTRY.

43. *Aricine, Quinine, Cinchonine.*—Aricine was obtained by Pelletier and Coriol from a variety of Peruvian bark. Pelletier infers from a comparison of his analysis of aricine, with the analysis of quinine and cinchonine by Liebig, that the three may be represented by three grades of oxidation of a peculiar